# Reply

GAD LEVY

*College of Oceanography, Oregon State University, Corvallis, Oregon*

CHRISTOPHER S. BRETHERTON

*Department of Atmospheric Sciences, The University of Washington, Seattle, Washington*

1 December 1988

The primary goal of our paper (Levy and Bretherton 1987, hereafter LB) was to critically examine the argument of Orlanski and Ross (1984, hereafter OR) in the light of semigeostrophic theory. We pointed out that the terms they identified as leading to a possible inviscid feedback mechanism limiting the collapse of fronts were very small in a semigeostrophic model compared to the ones which they neglected, and that semigeostrophic theory should be valid for roughly two-dimensional and inviscid fronts until the cross-frontal temperature gradients are concentrated in a region far tighter than that found in OR's simulations, at which point the Richardson number can become locally small enough to produce Kelvin–Helmholz instability. These ideas are not new; they were pointed out by Hoskins and Bretherton (1972). We evaluated the indirect viscous effects on the ageostrophic term near the surface in a realistic balanced boundary layer, and investigated the possible role of nonlinear terms in the divergence equation. Finally, we speculated that gravity waves produced during frontal collapse (Ley and Peltier 1978) could lead to the phase shift between vorticity and convergence observed by OR.

In his comment, Garner (1989) elaborates on the latter point. He suggests that inertial oscillations or long-period inertia gravity waves should dominate the wave response near the surface, because shorter period waves propagate energy vertically too fast to maintain large amplitude in the boundary layer. The appropriate forum for testing this idea is a two-dimensional numerical model with high resolution near the front, such as the inviscid Eulerian model used by Gall et al. (1988) or the inviscid Lagrangian model of Garner (1989). A shortcoming of both of these models is that neither model can account for the effects of Kelvin–Helmholz instability due to the vertical shear in the along-frontal velocity. Nevertheless, both models produce a frontal collapse to a scale limited only by the model resolution followed by the formation of a near discontinuity in temperature in the first few hundred meters above the ground. Semigeostrophic theory as extended by Cullen and Purser (1984) describes the essentials of this process well. While Gall et al. observed a small phase shift of 20 km between the vorticity and convergence maxima at the ground, and short wavelength gravity waves were evident in their simulations, both of these physical effects seem to be secondary to the basic collapse process. Neither Gall et al. nor Garner's numerical studies indicate that long-wavelength inertia-gravity waves play a significant role in frontal collapse, though it is unclear how to diagnose a "gravity-wave component" to the circulation in such a rapidly varying and horizontally inhomogeneous environment.

Instead, it seems more fruitful to examine how surface fronts are affected by dissipative processes in the planetary boundary layer (PBL). Williams (1974) and Keyser and Pecnik (1987) included frictional terms in their numerical models of frontogenesis and concluded that these frictional processes ultimately limited the frontal collapse and could lead to a steady state front. However, neither model included a realistic, moist PBL affected by surface heat and moisture fluxes. One must add complicated dissipative terms in the equations for the vorticity, $q$, and the surface horizontal divergence, $D$, at the surface as well as consider the indirect effect of the dissipation on the ageostrophic circulation and the nonlinear divergence response. In LB we pointed out that if only the indirect effect of dissipation on $q - q_g$ is considered, then dissipation could not help sustain a mechanism such as that of OR, because according to a similarity model of a baroclinic boundary layer (with thermal mixing, stratification and secondary circulation effects included), it is almost never the case that $q$ is supergeostrophic in the PBL, assuming it is geostrophic above the PBL. However, as mentioned in LB (p. 3415) and as Garner notes in his comment, the dissipative forcing terms cannot be neglected in determining the evolution of $q$ and $D$. Once again, it seems that numerical experiments coupling a boundary layer model to inviscid flow above would be the next logical step in modeling the wealth of different collapse phenomena observed in atmospheric surface fronts. Meanwhile, it is important to observationally assess down to what length scales geostrophic balance of the along-frontal winds, two-dimensionality and neglect of curvature are good approximations, and how these de-

---

*Corresponding author address:* Dr. Gad Levy, College of Oceanography, Oregon State University, Corvallis, OR 97331-5503.

pend on (and interact with) the nature of the PBL underneath (Reeder 1986; Fleagle et al. 1988; Levy 1989). One cannot underestimate the role that observations should play in corroborating numerical, theoretical, and analytical results; an invicid behavior of surface atmospheric phenomena can be regarded as physical only as a limit of the viscous behavior. The validity of semigeostrophic theory in two and three dimensions (Hoskins and Bretherton 1972; Cullen and Purser 1984) depends on observational estimates of typical background geostrophic deformation and on the scaling assumption that turbulent mixing sets in before ageostrophic accelerations become large. Scaling assumptions in the boundary layer solutions which permit the neglect of the twisting terms above the surface (Levy and Bretherton 1987; Levy 1989) rely also on observations and may break down at smaller scales, allowing the conversion of horizontal into vertical vorticity. Observations and simulations point to the possibility that different physical processes may control baroclinic waves and fronts under different conditions and at different stages. The existence and dominance of such processes in nature may be established only by careful observational studies which are well coordinated with theoretical and properly initialized numerical simulations with a proper boundary layer representation.

## REFERENCES

Cullen, M. J. P., and R. J. Purser, 1984: An extended Lagrangian theory of semigeostrophic frontogenesis. *J. Atmos. Sci.,* **41,** 1477–1497.

Fleagle, R. G., N. A. Bond and W. A. Nuss, 1988: Atmosphere–ocean interaction in mid-latitude storms. *Meteor. Atmos. Phys.,* **38,** 50–63.

Gall, R. L., R. T. Williams and T. L. Clark, 1987: On the minimum scale of fronts. *J. Atmos. Sci.,* **44,** 2562–2574.

Garner, S. T., 1989: Comments on "On a theory of the evolution of surface cold fronts," *J. Atmos. Sci.,* **46,** 1872–1873.

Hoksins, B. J., and F. P. Bretherton, 1972: Atmospheric frontogenesis models: Mathematical formulation and solution. *J. Atmos. Sci.,* **29,** 11–37.

Keyser, D., and M. J. Pecnick, 1987: The effect of along-front temperature variation in a two-dimensional primitive equation model of surface frontogenesis. *J. Atmos. Sci.,* **44,** 577–604.

Levy, G., 1989: Surface dynamics of observed maritime fronts. *J. Atmos. Sci.,* **46,** 1219–1232.

——, and C. S. Bretherton, 1987: On a theory of the evolution of surface cold fronts. *J. Atmos. Sci.,* **44,** 3413–3418.

Ley, B., and W. R. Peltier, 1978: Wave generation and frontal collapse. *J. Atmos. Sci.,* **35,** 3–17.

Reeder, M. J., 1986: The interaction of a surface cold front with a prefrontal thermodynamically well-mixed boundary layer. *Aust. Meteor. Mag.,* **34,** 137–148.

Orlanski, I., and B. B. Ross, 1984: The evolution of an observed cold front. Part II: Mesoscale dynamics. *J. Atmos. Sci.,* **41,** 1669–1703.

Williams, R. T., 1974: Numerical simulation of a steady state front. *J. Atmos. Sci.,* **31,** 1286–1296.